February 8, 1949

Dr. Max Delbrück
Kerckhoff Laboratories
California Institute of Technology
Pasadena, California

Dear Max,

I was delighted to get your card and appreciate very much your comment "a beautiful lecture" for, as you know, I have great admiration for your own and for your judgment in such matters.

I am particularly pleased to know that you plan to pursue further the theoretical analysis of the mutual inhibition hypothesis. It certainly needs to be done. Perhaps it will also be helpful to those of us who want to put the theory to a test. I have a few ideas about experimental tests and will get to work on them as soon after the Harvey Lecture as possible.

Perhaps you have heard by the grapevine, as you usually do, of the results of my latest tests of the plasmagene hypothesis in relation to the Paramecium antigens. I devised what I considered a critical experiment to see whether one gene could maintain the plasmagene presumably produced by an allelic gene. The result was negative and I am inclined to conclude that this means that when the critical test of mutability is applied to the assumed plasmagenes, the concept does not hold up. Of course, this is limited only to the antigen case and has nothing whatever to do with non-gene initiated cytoplasmic characters like kappa or plant plastids.

I am well aware that more than one interpretation of my result is possible. Either gene initiated plasmagenes do not exist in this case or such plasmagenes do not have the property of mutability, or the relation between gene and plasmagene is so delicate that even a small change in the plasmagene makes it impossible for the corresponding gene to maintain it. My present inclination is to conclude that this experiment relegates the gene initiated plasmagene to the realm of the non-demonstrable and, therefore, uninteresting concepts, but I still see alternatives and want to ponder over them for a while before committing myself in print. What is worse, the very fact of the dependence of the two antigens in my study on allelic genes is not in an entirely satisfactory condition. The genetic results depart slightly, but significantly, from the expected 1:1 segregation in the F2 generation. Until I fully understand what this means, I want to delay publication.

Since you have become so closely involved in what we are doing, I thought you would be interested in getting this information. I hope you will keep me informed likewise of your developments on the theoretical end, for I believe our experimental possibilities make the pooling of our efforts very worth-while.

With best regards to you and my other friends at Cal Tech,

Cordially yours,

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